



Discussions on Laboratory Testing of Backwards Erosion Piping of Soil: An Interview with John H. Schmertmann and Frank C. Townsend

By Bryant A. Robbins

PURPOSE: The U.S. Army Corps of Engineers initiated a new research project in Fiscal Year 2014 to expand the current state of knowledge regarding the backwards erosion piping potential of soils. As part of this project, laboratory testing is planned to investigate the piping potential of soils that have not yet been tested in previous research as identified in a literature review on the subject of backwards erosion. In preparation for U.S. Army Engineer Research and Development Center (ERDC) to begin piping experiments, Drs. John H. Schmertmann and Frank C. Townsend, both formerly of the University of Florida, were interviewed regarding their laboratory work on piping that occurred in 1981, 1986, and 1995. The purpose of this technical note is to document this interview for use by the engineering profession. A brief history of research on backwards erosion piping and a summary of laboratory testing that has been conducted prior to the year 2000 are included in this technical note to provide information for understanding the context of the interview. Additionally, an overview of the planned laboratory test program is provided.

INTRODUCTION: Backwards erosion piping, historically called simply “piping”, refers to a process in which the detachment of soil particles occurs at a free, unfiltered seepage exit on the downstream side of an embankment or other water retention structure. Once erosion has initiated, particles may continue to gradually erode until a “pipe” has formed continuously beneath the structure, leading to possible collapse and subsequent failure of the structure. The process of backwards erosion piping is illustrated in Figure 1. As shown, the ejected material at the unfiltered exit often results in the formation of a sand boil if the exit is vertical.

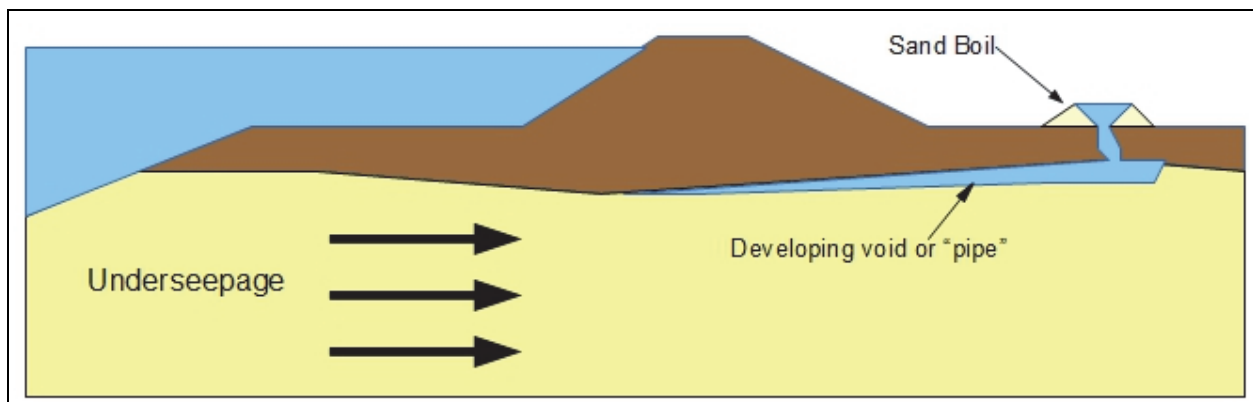


Figure 1. Illustration of backwards erosion piping with a vertical exit condition.



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The first reference to piping as a failure mechanism stems back to India during the late 1890s. For the latter half of the 19th century, British engineers spent time and effort constructing an elaborate network of canals and weirs in India for irrigation purposes. The Indian rivers typically consisted of deep sand beds upon which concrete and masonry weirs were constructed. As such, failures due to piping were frequent. One such failure at Khanki Weir in 1895 prompted Colonel John Clibborn to conduct model experiments on the Khanki sands. These experiments led to the recommendation that the average hydraulic gradient along the structure serve as the basis for design (Bligh 1910; Khosla 1930, 1936; Richards and Reddy 2007).

While the use of average hydraulic gradients contributed to a design method, the focus of the designers was on the uplift pressures that were exerted on the structure. Because this neglected the mechanism of erosion, failures continued to occur. It was not until 1910 that the first formal method for designing against piping was published. William George Bligh was a British engineer for the Royal Navy who also spent time in India during the late 1800s. Bligh was extremely familiar with the hydraulic gradient method as well as the numerous weir failures that occurred during his tenure in India. In particular, the Narrora Weir left an impression on him and led to his identification of a piping failure mechanism in his widely cited paper published in 1910. Bligh (1910) stated:

The safety of a structure subjected to water pressure and founded on a porous stratum from being undermined by a gradual process of washing out of the particles of sand by the percolating under current – termed piping – is thus clearly dependent on the length of the enforced percolation provided.

With this concept in mind, Bligh defined the quantity called the percolation coefficient as

$$c = \frac{L}{h_{cr}} \quad (\text{Eq. 1}) \text{ (Bligh 1910)}$$

where L represents the total length along the contact between the structure and the foundation, and h_{cr} represents the greatest differential head that could be applied across a structure of given length L before failure due to piping would occur (Bligh 1910; Terzaghi et al. 1996). Based on Bligh's experiences in India, percolation factors were recommended for various classes of sand and gravel as illustrated in Table 1.

Table 1. Percolation coefficients for sand and gravel (Bligh 1910).		
Material Description	Class	c
Fine silt and sand as found in Nile River	A	18
Fine micaceous sand as found in the Colorado and Himalayan rivers	B	15
Ordinary coarse sand	C	12
Gravel and sand	D	9
Boulders, gravel, and sand	E	4 to 6

Lane (1935) published an extensive paper in which he revised the rule of Bligh to be less conservative. In order to revise Bligh's rule, Lane reviewed 278 dam case histories, some of which failed due to piping. From this review, Lane noted that horizontal seepage paths appeared to have less weight than vertical seepage paths when computing the creep ratio for the structure. As such, Lane suggested using the weighted creep ratio (C_w) in place of Bligh's percolation coefficient, where C_w is computed as shown in Equation 2 with seepage lengths taken as shown in Figure 2. Additionally, Lane established recommended values of weighted creep ratios for various soil types as shown in Table 2.

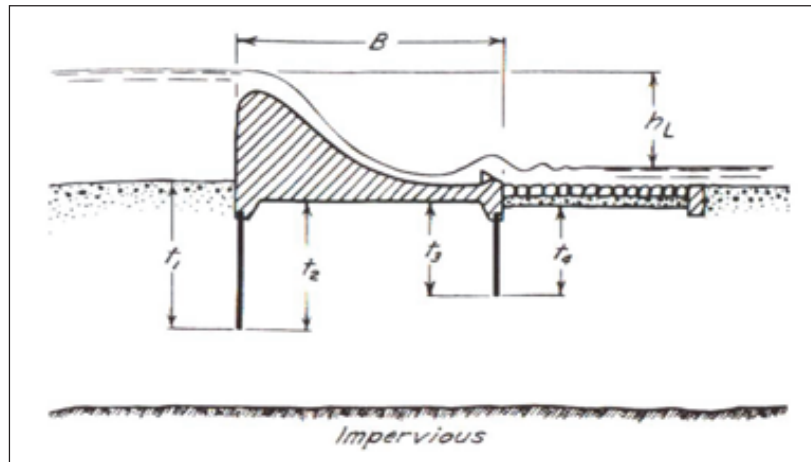


Figure 2. Illustration of dimensions for computing creep ratio (from Terzaghi et al. 1996).

Table 2. Weighted Creep Values (Lane 1935).	
Material Description	C_w
Very fine sand or silt	8.5
Fine sand	7.0
Medium sand	6.0
Coarse sand	5.0
Fine gravel	4.0
Medium gravel	3.5
Coarse gravel including cobbles	3.0
Boulders with some cobbles and gravel	2.5

$$C_w = \frac{\frac{1}{3}B + \sum t}{h_{cr}} = \frac{\frac{1}{3}B + t1 + t2 + t3 + t4}{h_{cr}} \quad (\text{Eq. 2}) \quad (\text{Lane 1935})$$

where:

B = horizontal seepage length
 t = vertical seepage length
 h_{cr} = the critical head across the structure

While the rules of Lane and Bligh provided a basis for assessing structures, they were based on empirical data that could not be used to determine, with known certainty, whether or not a structure was safe. A structure could easily be founded on much worse conditions than any of those included in the data sets of Lane and Bligh, thereby yielding an unsafe structure despite the computed creep ratios indicating otherwise. Likewise, a structure could be founded on much better conditions than the structures in the empirical data sets leading to an overly conservative design. Terzaghi (1929) quickly recognized these shortcomings and illustrated how these empirical design rules cannot account for site-specific factors. Perhaps part of the reason the simple, empirical rules gained use so quickly is because seepage analysis was uncommon at this point in time. While flownets were first published by Forcheimer in 1919 (Terzaghi 1922), the method of constructing flownets was not thoroughly described in English until Casagrande (1937). Even then, it still would take years before flownets began to see widespread use in America. In 1948, Terzaghi and Peck published the first edition of *Soil Mechanics in Engineering Practice* in which the concept of “critical gradient” was clearly described (albeit, only vertical critical gradient was discussed) as the ratio of gravitational forces to upward seepage forces. Additionally, Terzaghi and Peck (1948) recognized that a filtered exit is all that is needed to prevent piping from occurring as it increases the gravitational forces without increasing the seepage forces.

20TH CENTURY LABORATORY TESTING OF PIPING: Terzaghi and Peck (1948) found that the ideal solution to backwards erosion piping was to provide a weighted filter at all exits. Unfortunately, this knowledge was not available at the time of construction of many flood defense systems. Therefore, it was (and still is) highly desirable to be able to predict the response of structures with unfiltered exits during flooding. With the advent of numerical solutions for assessing groundwater flow in the latter half of the 20th century, it became possible to assess site-specific seepage regimes for the purpose of assessing the gradients that may exist under a structure. To evaluate the potential for backwards erosion piping, the critical values of local seepage gradients for particle transport must be known. The desire for this knowledge resulted in two major laboratory investigations during the 20th century as described in the following sections.

DUTCH LABORATORY INVESTIGATIONS: The vast majority of the Netherlands is protected from either the river or sea by clay dikes founded on sandy subsoil. These conditions are extremely vulnerable to the backwards erosion piping failure mechanism. As such, an extensive research program was started in 1972 to study this process. As part of this investigation, laboratory tests were carried out between 1973 and 1983 at three different scales (Weijers and Sellmeijer 1993). Altogether, 34 small-scale tests, 14 medium-scale tests, and 23 large-scale tests were conducted (Schmertmann 2000). One of the small-scale configurations is illustrated in Figure 3.

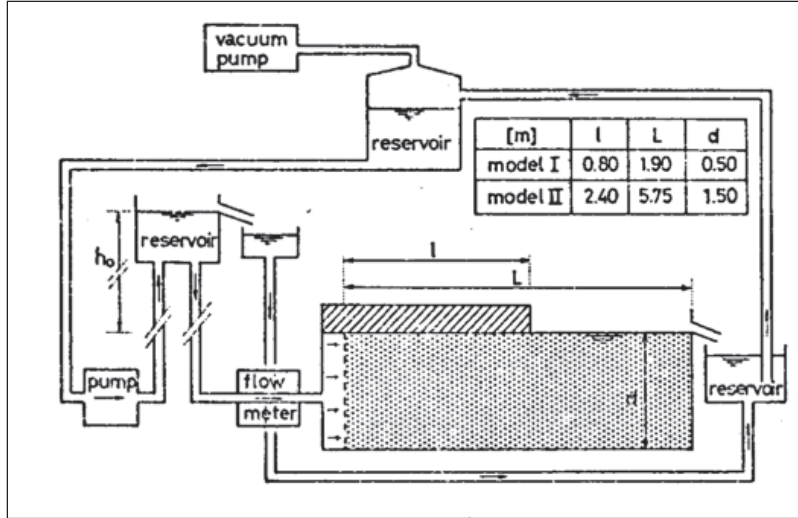


Figure 3. Configuration of small-scale laboratory test conducted by the Dutch to study backwards erosion [dimensions in meters]. (de Wit et al. 1981).

With the resulting observations of these 71 tests in hand, Sellmeijer (1988) formulated the first mathematical model to describe the observed phenomena of piping. The mathematical model was based on a simplified description of the problem and revealed that the critical head occurs when the piping length reaches approximately halfway under the structure. Before the pipe reaches this point, the increased permeability in the piping channel is much greater than the surrounding soil, and the near-surface hydraulic gradients are damped, thereby allowing equilibrium to be reached (Sellmeijer and Koenders 1991). Their study of the problem showed that the critical gradient can be solved by setting the pipe length equal to one half the total seepage length. The resulting equation is given as:

$$\frac{H_{cr}}{L} = G R S F \quad (\text{Eq. 3}) \quad (\text{Sellmeijer 2006})$$

where:

$$G = \text{geometry factor} = G = \left(\frac{D}{L}\right)^{\left[\left(\frac{D}{L}\right)^{0.28} - 1\right]}$$

$$R = \text{rolling equilibrium} = \frac{\gamma'_p}{\gamma_w} \tan(\phi)$$

$$S = \text{sand properties} = \eta \frac{d}{\sqrt[3]{kL}}$$

$$F = \text{force factor} = F = 0.68 - 0.1 \ln(S)$$

$$D = \text{aquifer height}$$

$$H_{cr} = \text{critical hydraulic head}$$

$$L = \text{dike width}$$

- γ_p = unit weight of particles
- γ_w = unit weight of water
- θ = bedding angle
- η = Whites constant
- d = particle diameter
- k = intrinsic permeability

This equation for critical head has been calibrated to the many laboratory tests and remains the design standard for dikes in the Netherlands. However, the equation was developed for very specific conditions and should not be extrapolated beyond the conditions for which it was developed. That is, this equation was developed for the case in which seepage is exiting through a two-dimensional slot in the confining layer as would be present in the case of a ditch penetrating through the clay confining layer. Furthermore, the testing upon which this method is calibrated used sands with a uniformity coefficient ranging from 1.58 to 3.53 and a D_{10} of 0.28 mm or less. The validity of this equation beyond these limits is largely unknown.

UNIVERSITY OF FLORIDA INVESTIGATIONS: In 1979, a dike failure in Martin County, FL, released 33 billion gallons of water over portions of the county (Townsend et al. 1981). As a result, a special consultant board was convened to investigate the cause of failure [Dr. Schmertmann was a member of this board]. The board concluded that the failure was most likely due to piping. This investigation re-emphasized the fact that only empirical methods were available at this time for assessing the likelihood of backwards erosion piping. As such, Dr. John Schmertmann developed the first quantitative piping theory in 1980 (Townsend et al. 1981). Flume testing was conducted at the University of Florida to verify this piping theory. A schematic of the flume used for the testing is shown in Figure 4. Altogether, 39 tests were conducted as part of three studies conducted in 1981, 1986, and 1995. The tests were on clean sands with uniformity coefficient (C_u) ranging from 1.4 to 6.7, and D_{10} ranging from 0.062 mm to 0.80 mm.

The results of the flume tests largely verified Schmertmann's piping theory and led to the publication of an equation to calculate the no-filter factor of safety (FS) against piping. The equation is:

$$FS = \frac{C_D C_L C_S C_K C_Z C_Y C_\alpha i_{pmt}}{C_R i_f} \quad (\text{Eq. 4}) \quad (\text{Schmertmann 2000})$$

where:

- C_D = correction factor for thickness of susceptible layer
- C_L = correction factor for total pipe length
- C_S = correction factor for grain size
- C_K = correction factor for anisotropy
- C_Z = correction factor for layered aquifer with high permeability underlayer
- C_Y = correction factor for density
- C_α = correction factor for inclination of layer
- C_R = correction factor for embankment axis curvature

i_{pmt} = maximum point gradient required for piping in the laboratory
 i_f = point gradient expected to exist in the field

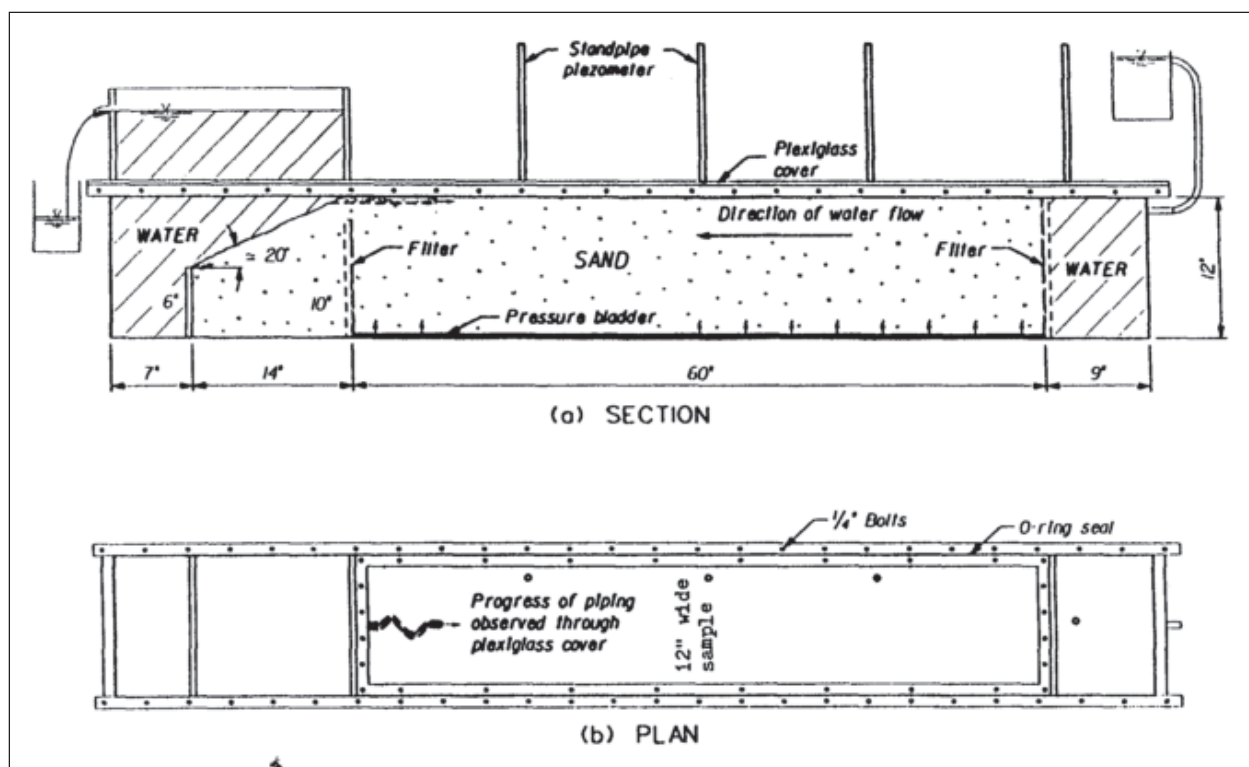


Figure 4. The University of Florida piping test flume [dimensions in inches] (Schmertmann 2000).

For a complete description of the correction factors, the reader is referred to Schmertmann (2000). It should be noted that the gradients referred to in this equation are “point” gradients just upstream of the advancing pipe as opposed to average gradients across the entire structure. The point gradient is the value of the local gradient in the flownet upstream of the pipe before the presence of the advancing pipe distorts the flow. This value can be solved for directly through the use of three-dimensional finite element analysis or numerous flownets as illustrated by Schmertmann (2000). Alternatively, the average (or global) gradient along the flow path can be corrected to approximate a point gradient, provided average correction factors are used that apply to the entire pipe length under investigation.

PLANNED RESEARCH: Both the Dutch and Florida research on backwards erosion contributed greatly to the profession’s understanding of conditions that may cause backwards erosion piping in clean sands. However, questions continue to arise during risk assessments of the Corps’ dams regarding the piping potential of soils that contain particle sizes much larger than sand and soils with uniformity coefficients significantly larger than the maximum value of 6.7 tested to date. As such, ERDC has undertaken a laboratory research program to conduct further flume tests. The research involves flume testing on soils ranging from fine sand to fine gravel. Additionally, soils with coefficients of uniformity ranging from 1.6 to 20 will be tested. The goal of this research is to assess the validity of previously developed theories beyond the range of data for which they were developed.

In order to conduct further flume testing, ERDC is designing multiple laboratory flumes. In an attempt to gain as much insight as possible regarding this type of testing, Drs. John Schmertmann and Frank Townsend were interviewed about their experiences with the laboratory testing of piping and the phenomena of piping in general. This interview provided significant insight into the testing procedures used. Furthermore, the interview was extremely instructive regarding the process of piping through elaborate descriptions of the interviewees' observations over years of laboratory testing. The interview has been transcribed and attached to this report as Appendix A for future reference.

CONCLUSION: This report has documented a brief history of backwards erosion piping and the laboratory testing thereof to provide the reader with a background for understanding the interview transcript attached as Appendix A. Preserving the attached interview between the author and Drs. John Schmertmann and Frank Townsend regarding backwards erosion piping was the primary purpose of this technical note, as the interview provides significant insight into the historical laboratory testing of piping conducted in the United States. This interview should prove useful for others who attempt to conduct similar testing in the future.

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APPENDIX A – INTERVIEW TRANSCRIPT

The following is an [annotated] transcript of an interview between Bryant Robbins (BR) of the U.S. Army Corps of Engineers and Drs. John Schmertmann (JS) and Frank Townsend (FT), both retired from the University of Florida. The interview was conducted in November 2013. The transcript below appears in its original form with only minor editorial changes.

BR: You did approximately 15 years of flume testing [to study piping], correct?

FT: We started in about 1981 when we did the Martin County Dike Failure, and then we wrapped it up probably about 1987 or so when we finished up activities for the USBR [U.S. Bureau of Reclamation]. There was down time in between. We finished the Martin County Dike, and then it [flume testing] went to rest and nobody was interested. Then the folks at Denver, the USBR (Dwayne Campbell), got excited and resurrected the flume. We did about three years' worth of work for them and then it died again. That was the last involvement I had. The last thing I did was in 1987.

JS: That sounds about right. Then we had another dike failure—a tailings dam failure. If I had to guess, I'd say we did another nine tests with the flume. We resurrected the flume, brought it back up to speed, and did some tests as part of a consulting job for the dike failure.

BR: Was that the 1995 work?

JS: Yes. I think we paid the university [University of Florida] a fee for that.

FT: I think we got a rental fee, but it was somebody from Schmertmann and Crapps that came over and did the test because I don't think there was a student involved as we didn't get a dissertation or a thesis out of it to the best of my knowledge.

JS: There was a master's student who we [Loadtest Inc.] eventually hired.

FT: Okay.

BR: Were the scour tests done as part of that consulting work?

JS: Yes.

BR: What do you think was the most meaningful item or items learned from all of the lab testing you conducted?

FT: From my viewpoint?

BR: Yes, both of your viewpoints.

FT: Now, remember that I'm much less theoretical with stuff like this; here is a quick, basic summary from my viewpoint. If the average gradient gets to 10-15%, you're going to have trouble. So basically, I just look at the average gradient. That was what I learned and the data are there to show that. The second thing was this: You can kill piping with a graded filter. It will flat stop it in a heartbeat. We tried desperately to breach filters, and we couldn't breach them. So, in summary, the two lessons that I learned from it are — if the average gradient gets to 10-15%, you better start thinking about piping. If you want to kill it, you can kill it with a graded filter without any trouble at all. We never got to try geotextiles, which would have been an easier approach now with today's equipment. But back in those days graded filters were all we knew. And, the graded filter criteria worked perfect. If you use a D15/D85 of 4 or 5, it works perfectly. We even tried to fail it with a filter. We made some that were out of specs and we couldn't breach them either. We finally got some that were really, really out of specs that failed. I don't remember the numbers exactly, but they are in the report [(Townsend and Shiau 1986)]. For these, the sand went right through the filter. We did a series of tests where we said, "Okay, we

are going to violate the filter code to see just how far we could push the limit,” and we were able to push it. You can breach a filter if you get the gradings off so badly, but it has to be way off. That’s basically what I remember from the test.

JS: I agree. Let’s see, what did we learn? We learned a lot.

1. First of all, we were surprised that the pipes had the braided shapes that they did. We expected a little bit of waviness in the pipe, just anticipating, but it was much more dramatic than that. It was almost like tree limbs with a lot of branches, some of which then clogged, and new branches would then form. You get a very braided type of behavior. Almost an oxbow, you might say with, sometimes, actual oxbows that formed and were then short-circuited just like when a lake is formed. It’s quite different than the straight path you might assume. However, that doesn’t mean you can’t use a straight path approximation for the analysis. That was a surprise. We found out that the people in Holland had the same surprise [(de Wit et al. 1981; de Wit 1984; Sellmeijer 1988)].
2. The next item was the importance of the coefficient of uniformity. We never expected that. We didn’t try to anticipate it... we just looked at the results and, WOW! And then we found out that the people in Holland had essentially duplicated our results, at least fitting their data into our graphs more or less matched that is. When you are getting relatively clean sands, the coefficient of uniformity has a dramatic effect. We used the D_{60}/D_{10} because everyone understood that; it was convenient, so we used it! That doesn’t mean it’s the best ratio to use. It was the only one we tried. It was so dramatic that we just used it, used the results, but you might find something superior. It looked so good to us that we didn’t think it was easy to find anything superior. But, you know it might be D_{70}/D_{10} or D_{70}/D_{20} . I think Sellmeijer in effect used D_{70}/D_{10} , but anyway that’s another thing we learned.
3. I think we confirmed that it is the very high gradients at the pipe head that causes the pipe to advance. Frank here, he described it, and I quoted your description in my paper [(Schmertmann 2000)], that it’s a series of little landslides. A kind of flow slide at the end of the pipe end that gets washed away; and, that behavior inches forward in a series of increments, a series of mini-slides you might say. Then, that sand that has sloughed down because of the high gradients washes away. It’s essentially a slope stability problem. I mean, if you have a slope and you get the gradient high enough, the stable slope is horizontal. So, that disappears and the gradients that develop at the end of the pipe are whatever it takes to start moving this stuff because you have a three-dimensional convergence. If you have a pinhead convergence, you are getting very high gradients; and, of course, things have moved before that so it’s almost a self-limiting size depending on the gradient and rate, rate of movement. That’s another point. You can describe it by either of the two mechanisms. One is a quicksand-type action at the end of the pipe and the other is a sloughing of the slope. I think it is probably some combination of the two, but I think either one by itself will describe the process. I chose to use the quicksand approach. For one thing, it is more dramatic, just the word. I thought about quicksand behavior at the end of the advancing pipe. The other is I thought the mathematics of the problem were just simpler. It was simpler to explain it, but I think either one of them would have worked.

- a. BR: You believe the two [mechanisms] are related to the point that they both are adequate approximations?
 - b. JS: Yes, I think so. As a way of describing it, at least.
4. I had quite a conversation with Professor Ralph Peck about piping problems. I spent a couple of hours talking to him about it, and I realized that the piping problem to most engineers is the sand boil problem that you mentioned. It's the vertical flow, the upward flow of water, to form a quicksand boil. That's what they think of when they talk about piping. I tried to explain to Ralph Peck that that's just a special case. Piping can occur in any direction. I tried to convince him of that, but he had a mindset that was very difficult to call anything else piping that wasn't a boil type of piping. So, I learned that it's difficult. You have got to get people thinking in terms of pipes occurring in any direction. By the way, Peck was very much impressed with the coefficient of uniformity. He thought that was a clear contribution because nobody had ever noticed it before. He wasn't so convinced about piping in any direction, though.
5. Another is this: I have discovered since then that transient flow can cause pipes. The first time a reservoir is filled, transient flow gradients can start to create pipes. Especially if there is some place where they can discharge during the transient flow.
- a. BR: How did you make that observation or discovery [transient flow piping]?
 - b. JS: The dam failure in 1995, the IMC-Agrico failure. That was a condition where we think the pipes developed during the initial filling, but they didn't really complete the total paths until the final filling of the dam and the operation of a spillway. The spillway for the dikes just consisted of a large diameter metal pipe. It's when they started operating the spillway that the conditions changed enough so that the pipe completed in just a few hours...completed the path and it failed very quickly.

FT: In John's discussion, there are three things that sparked my interest; so, I'll go back and pick up on them. The one reason why I hang my hat on the average gradient is because I don't know the length of the initial pipe. I have no idea of knowing if I've got 2% penetration or 50% penetration due to just the construction or the way Mother Nature left it. So, that's why I've always hung my hat on the average gradient; it's the only thing I do know. The other thing that we found was important from the flume studies is you have to have a roof. In other words, if there's not a surface up above, it will collapse and there won't be pipes. You have to have a roof that sits at the top of the thing like that to make it work. So, those are some things that just came to my mind. The third thing is that John was describing the failure of the IMC dike, but another place for you to go look is your good friends in the [USACE] Jacksonville District. They have been plagued with good ole Hebert Hoover Dike. At Herbert Hoover dike, they have had the sand boil problems, too. I know your experience, being in Vicksburg [MS], is going to be the Mississippi Valley levees, but there is another case history down there that gets tested every hurricane when the level comes up. In my work that I did back in the mid-1990s for Herbert Hoover Dike for the Jacksonville District, I told them that once they got to elevation +18 ft, they

had better start thinking about piping because their culverts are just exactly what John described. They are just a corrugated metal pipe culvert that was put in, and that's the weak link. I have recently discovered that they are doing some work over there, and doing a lot of construction. So, Herbert Hoover Dike will give you some field studies if you want to look for field case histories.

BR: Do you think that Jacksonville District has field reports on their sand boils?

FT: I wrote a report over there for them; so, yes, it's somewhere. But, it's probably long since lost. Besides that, there's a lot of work being done in the district on the Herbert Hoover Dike.

BR: Yes, they are doing a lot of work. In fact, I actually had the opportunity to go over there for two of the foundation inspections on the culvert replacements. There was this limestone unit in the foundation that just consisted of cemented seashells.

FT: That's exactly what got us started. Martin County Dike is just an extension of Herbert Hoover Dike, which got our entire effort kicked off back in 1980/1981.

BR: There was a very interesting stratigraphy visible in the foundation excavations. I saw that seashell limestone I had mentioned, which was overlain by various layers of peat, sand, and shells. The sand was possibly the finest sand I had ever seen! It looked exactly like a fine sugar... I had never seen anything like that in nature before.

JS: Sands like that will pipe at a 5% gradient.

FT: Yep, they'll go at 5%.

BR: If you were to do flume testing activities again, would you do anything differently? If so, why?

JS: Before we leave it, you brought up a very important point—the roof. You have to have some kind of a roof. However, when you get out in the field and try to apply that in practice, you kind of scratch your head. What does a roof have to consist of? Does partially saturated sand have enough cohesion and stay in place so that you can develop a pipe under it and use the cohesiveness of the capillary tensions to reliably have a roof? I've often wondered about that. I don't know. That would be something very important to test because that happens all the time.

FT: We never created a pipe at depth. In other words, we always had the pipe right underneath the flume glass. If I were to create a pipe at depth, there are two ways of doing it. One is to use a simple dowel as we did and withdraw it; the other way we discovered to make a pipe was to use sugar. We would form a pipe with sugar and it would dissolve and disappear during the saturation. Then we had a natural pipe.

JS: Great idea! Yes! We never tried that. We only had horizontal pipes with the Plexiglas covers over them [in the flume tests].

FT: That's all we were trying to do at the time; so, we never tried to do a pipe at depth. In other words, you could test this roofing effect and see if it would collapse or not. We never got the opportunity to try it. It was more of an academic interest to do something like this, but it could answer the transient piping questions.

FT: Have you built a flume at WES [ERDC] now? Do you have a flume yet?

BR: We built a preliminary one out of wood just to start playing.

JS: What size are you going to make it?

BR: The one we have right now is 8 ft long, 18 in. wide, and 12 in. deep. There's an upstream/downstream configuration as you had. It's almost exactly, conceptually, what you had built. So, the sample length is about 6 ft.

FT: How did you solve the bladder problem - the bladder on the bottom?

BR: What we tried first was copying something the Dutch had done in the last ten years on some of their smaller tests. They put a really thick compressible seal on the top and just clamped it down and applied the confining stress through the clamps. What we are finding, though, is that doesn't work really well in this case because the sand arches at the top. So, we really need to push it up from the bottom as you did.

JS: Yes.

FT: Yes, one of the shortcomings in the bladder was once or twice it broke. The other one is that it tends to dome or arch as you've described. If the pipe ever got over on the outside edges, it would really run because it had less confinement there. I was thinking if you could have a bellow type of system with a plate, you could lift the whole plate up and make it uniform; but we didn't have that. The bladder was an ingenious idea. It allowed us to do what we wanted to do and without that contribution we would have still have been scrambling and watching sheet flow forever and ever. We never would have gotten the pipes to work without it.

JS: We also learned that effective stresses were not controlling the formation of a pipe.

FT: Yeah, no they don't.

JS: You know, my initial design was with a bladder. That seemed to work. I like the idea of 18 in. wide and 12 in. high instead of 12 by 12 because that makes the bladder pressure distribution a little more uniform in the area where the pipe is going to form, say in the central 12 in. of the top of the flume. Regarding the confining pressure, we learned that we'd get the same pipes whether we had 6 psi pressure on it to begin with or 1 psi pressure on it. It's an important question because people wonder how pipes can form if the effective stresses are so high down below a dam. How can you get a pipe forming with such high vertical effective stresses? The answer is that it's almost independent of the overburden pressure because it's the liquefaction that you get at the end of the pipe that controls, and the stresses just arch over this tiny little pipe.

BR: So, you get a stress relief.

JS: Yeah... oh, you might think also that a pipe is going to change your seepage patterns... I'll back up. Typically, you have a two-dimensional flownet drawn for the section. People wonder how a pipe is going to change that flownet, and the answer is very little. It is only local changes, and it's very local. We learned that the piping situation is a local situation. That is, the overall gradients through the dam are going to permit its progress or not permit its progress; but, the actual development of the pipe has very little effect on the overall flownet, three-dimensional flownet, for the dam.

BR: I saw you had estimated that region to be about 80 times the pipe radius.

JS: Yeah, something like that.

BR: Did you make that estimate from looking at flownets, the transverse flownets that you drew? [Schmertmann 2000]

JS: Yes, that's the idea. But, it also comes from tests done at the Dutch hydraulics labs. I guess the Delft hydraulics labs as they are now called. They had a very large flume. I mean really big! It was something like 12 ft wide if I remember correctly; and, they found that multiple similar pipes would form within such a large entry area, which clearly showed that it's not affecting the two-dimensional flow patterns. The flow pattern is not seriously affected by the location of a pipe or the occurrence of a pipe. They had three or four of them develop along the width of their flume.

BR: I see how the Dutch tests were illustrating an effectively two-dimensionally averaged behavior. They had a free continuous boundary condition on one side. But, I've been wondering about cases where the flow is forced to be concentrated. Like in your tests, you forced the

initiation with the starter pipe. Did it always start there or did you also see other little pipes form on the edges as well?

FT: Not always. To the best that I can remember, it always started where we forced it to start.

BR: It did always start at the head of the starter pipe then?

FT: I don't remember... unless we screwed up on the side like I mentioned. The side was always a problem, but it usually always started where we forced it because that was where everything would concentrate. I mean that was the weak link.

JS: And our flume was only 12 in. wide; so, there really wasn't much room. Clearly within 12 in. the pipe will affect the flow path, I mean the flownet, the 3-D flownet.

FT: But John is absolutely right. The bladder pressure had very little effect on it. I think we pushed the heck out of the bladder once, up to 10 psi, with no difference in results.

BR: The reports said that you had the bladder bolted to the flume bottom in a frame?

FT: Yeah, it was. A picture frame went around it, and it was just a rubber bladder. We had little ring seals on the screws that held it. I think today if I could do one and design flumes, I would have had a rectangular plate, and I would have a bellowed system such that the plate would come up and the bellows would keep it sealed down below so I would get a completely uniform pressure as opposed to the bladder that arched. Then I would always know what the pressure at the edges was going to be. We didn't have the time to do it... the picture frame idea was great, it was a key to make this whole thing work.

JS: What was the key again?

FT: The key was the bladder. If we had built the flume and put the top on it and tried to do it without the bladder, nothing would have happened. It would have been a sheetflow underneath it. That was the key to make all these experiments work.

JS: We knew that from the beginning. Our initial design had a bladder.

FT: I think when we first did Test No. 1 or Test No. 0, we didn't have the bladder in because I remember someone and I fussing about the bladder and having a conversation about trying to get it going to finally make the test work. The other thing is, there is a tradeoff. I am thinking as an experimentalist now. Yes, I like big flumes; but, you have got to remember the manpower to build a sample. It becomes a point. I mean Waterways [ERDC] is great; look at the 36 in. diameter triaxial test. I was there when we were trying to get that stuff going. That takes a whole week to do one test. That's a lot of bucks! So, the compromise that John came up with in the design of 12 by 12 was perfect. We could almost do a test...we were doing two tests a week and sometimes, a good time, we got three days a week, three tests a week. In other words, we would get the sample formed, we'd let it saturate overnight, and we're doing the test, tear it down, start the next test going, saturate it overnight, do the test, tear it down. The best production we had... we may have done a three-test week one time, but usually two tests a week was pushing it. You get these big flumes and you start trying to process material, etc., etc., etc. It can impede you. I mean it's great to have a great big flume but you are going to get one test a week and things drag out. So, keep that in your mind.

FT: I think our size was perfect because we could actually get productive. We did a heck of a lot of tests.

JS: I think we had over 30, maybe 40.

FT: We could get tests done and the student could look at the end of the tunnel and know that he was going to graduate.

JS: Well, that's it, too... you are talking about students doing these tests. So that's production with students who are not putting full time into it.

JS: Thirty-seven tests I've got down here that we did. At least I could find records of 37 tests.

FT: That's production, and that's allowing you to get data points to fill in the gaps as opposed to getting one data point a week.

BR: Part of the reason we chose to go a little wider was we wanted data on some soils that contain gravel.

FT: That's understandable.

BR: But right now, we are also thinking about even smaller than what you had as well to play with the uniformity coefficient in more detail through a sand and silt mixture.

FT: No, no, no... you keep your big flume, but you partition it. In other words, you don't have to use the full 18 in. if you don't want to.

JS: Start using coarse sands and gravels and you're going to have a lot of water to deal with. And you might get into turbulent flow problems. I don't know how this whole piping idea applies then because it's based on flownets, which assume laminar flow.

FT: Practicality is an issue if you want to look at gravels, and that's why we did 36-in.-diameter triaxial tests; we wanted to look at earth/rock mixtures. I'm a modeler, I admit, and I am biased. I have no trouble doing scalping and replacing and shifting gradations down, but that's because I am a centrifugist and modeler. I have no trouble doing that. I'm not sure whether that's going to answer your critics if you scale down from a 1-in.-diameter particle down to something else. But, I would probably use the smaller flume first to answer my question if I could get away with it. One-sixth has always been my rule of thumb [for ratio of maximum particle diameter to test sample dimension].

BR: Okay.

FT: But I have been forced to scale because I have done centrifuge work and in the centrifuge I've got to scale. I don't have any choice.

JS: What is this 1/6 thing?

FT: The diameter to the size of the particle. So, in other words, if you wanted to test a 1-in.-diameter gravel, you need a minimum of 6-in.-diameter specimen.

JS: Okay, okay...

FT: So, if you go to 3 in., now you are looking at a minimum of 18 in.

BR: With the bladder you said that water was found to be better than air. Was that simply because of the compressibility, and you were trying to measure volume changes?

FT: I just have always liked water, and I think it is because I am a triaxial guy. I don't like air in triaxial chambers. I think there was an apprehension on our part as to what would happen if the bladder leaked. With water, a leak is just putting more water into the system.

BR: Yeah, okay, I see.

FT: So, that was why. If the bladder leaked air, I have desaturated my whole system. That's probably why we went with the water.

BR: With the vacuuming and de-aired water, I saw you stopped vacuuming at some point because it wasn't working.

FT: Yes, that was a waste. The vacuum never worked.

BR: Did you also stop with the de-aired water?

FT: We tried, yeah, but you can't de-air enough water.

BR: Okay, I wondered about that.

FT: We gave up with that. I think we tried that at the beginning and we gave up with it. Saturation was a real problem, and I think we pretty much gave up trying. We found that it really didn't make a big difference.

BR: Okay.

FT: It contributed more problems than it did help; and yes, there was trouble where we would see bubbles would happen and we'd just say "Okay, we've got bubbles in the system. So what?" That's a consideration; but, that's a hoop that you need to jump through once or twice to try to prove it to yourself. I would abandon the de-aired water right off the bat. That's a misnomer. We would vacuum overnight and we would vacuum the sample over night, put vacuum pumps on both ends of the sample and just vacuum as much air as we could out of it. Then we would start the process of bringing the water in. But I'm a triaxial guy, I've always used, for saturating sands, I've always used vacuums. It's the only way to go. That's what I tried to do.

BR: I noticed the notes on some of the tests mentioned air bubbles would block a pipe here or there but it would just end up going around it anyway. You didn't think those made a big difference for the test results?

FT: We tried, we tried to overcome that, but it was the best we could do.

BR: Okay. So, all the tests in the literature, the sand's in direct contact with the Plexiglas?

FT: Yes.

BR: Do you think that's different enough from the field to cause significant error in the measured critical gradients?

FT: You've got to have a roof so I would... As I mentioned before, it would have been neat if we had formed the pipe 2 in. below to see what would happen. We are talking about putting the pipe below the Plexiglas.

JS: Yeah.

FT: Obviously, you are not going to see anything unless it comes up, but it would have been cute to try. We never tried it. It would have been a nice test to do. Probably should have tried it just for grins, but we didn't do it. I thought it would have been a good idea to try it.

BR: Do you think though that the sand/Plexiglas contact causes a higher seepage than you'd see if it was something like clay on top? Enough to really matter, that is?

JS: I don't know. It never seemed to matter. When you look at it visually, you can sort of see where the contact with sand is when you push it up. It wasn't perfectly uniform but it gave kind of a uniform look to it.

BR: Okay.

FT: The whole purpose of the Plexiglas was to see. I mean, we wanted to see what the heck was going on.

JS: Well, of course.

FT: That was the whole purpose of the Plexiglas. I don't really think it made a big difference. You could resolve that problem quickly by forming a pipe at depth and see what happens. I mean that would answer your question.

BR: I noted that some of the Netherlands tests had clay slurry put there to try to get a better transition. Yet, the tests seemed to compare very well with what you had done despite the different contact.

JS: Yes, also the Dutch tests; they didn't use a Plexiglas. They used a different kind of a contact and their results fit in with ours. I don't think it made that much difference.

FT: I don't think it makes that much of a difference either.

JS: It may make a difference in how the path actually wanders. If you don't have completely uniform contact, it may wander in a different way compared to if you had, but the path itself seeks the least resistance to where it can expand.

FT: I mean, getting down to minutia details now, it depends on how well you screeded the sand off to begin with. In other words, if you were sloppy in the work, you would create a weak zone and that would tell the pipe “Say, hey, I’m going to that place there.” Like I said before, if it ever got to the edge because of the bladder problem, it would go down the edge. I mean that was the path of least resistance and that’s where it would take off.

BR: I wanted to ask about the screeding as well because we have found that even with raining the sand, it was still uneven enough that we would have to go back and fill it in by hand.

FT: We did hand screeding, and we did hand patching. You would screed it off and try to get it close and then you would go back and patch it. I mean if you felt that you messed up, you could go back and patch it.

JS: Did you rain your sand into your tests?

FT: Oh yeah, we rained it all.

JS: We didn’t rain any.

FT: No, no, we rained...the tests we did, John, we rained it.

JS: Yeah, but I’ve done tests also.

FT: Oh, you didn’t rain it?

JS: No.

FT: Oh, you just shoveled it in?

JS: No, I did a lot of tests personally right at the very beginning and in 1995. No, we didn’t rain in it.

FT: We rained all the sand in.

BR: How did you place it?

JS: Just as uniformly as we thought we could under water.

BR: Okay, we are using water...so you air pluviated [question to Dr. Townsend]?

FT: We air pluviated.

JS: But under not much water... just a little bit.

BR: Yeah, I see what you’re saying so...

JS: Just to keep it saturated, that is.

FT: We air pluviated, and we had shutter plates. So, in other words we had a shutter plate, and then you would open the shutter to let it start raining. Basically, if you go back to Paul Gilbert’s [Paul Gilbert was doing research on liquefaction] work at Waterways [ERDC] on forming uniform samples, he had a rule of thumb about if you dropped it more than 12 in. it was okay, or something like that, for pluviating. I think our height drop was at least 12 in. I know it was. That was the thing because you had plastic all over the floor because you are getting it raining outside the flume and so we were always trying to scoop that up. It was a lot of work. John’s method just pouring it under water was a heck of a lot simpler. I don’t know. I don’t have the answer to “does it make a difference or not.” I don’t really have an answer to that question.

BR: Okay. We are trying to water pluviated it just to simplify our saturation process.

FT: Yeah, if you water pluviated, all you have to do is just drop it. You can go with a shovel and just drop it in.

JS: That’s kind of what we did, a little more carefully than that. You know, we tried to get uniformity. We would take a small scoop and spread it around.

FT: I think you could probably pluviated it through water with just a couple of inches and you’d get the terminal velocity and I think it would be perfect. You are just going to have to let the water spill out over the top. I mean you’re bringing up the water to the top, and it just goes

outside the flume. That'd probably be an easier way to form your samples than in a vacuum. I think that's a better idea. Just pour it under water, just pluviate it.

JS: Don't drop it too far or you'll get segregation.

FT: Right.

BR: I wondered about that, too. When we go into these broadly graded soils that have a higher fines content...

FT: Then you can't pluviate through water, you'll segregate it.

BR: Do you think placing and compacting would yield a sample uniform enough for these tests, or is it going to lead to significant density variation on the placement lifts?

JS: You'll have to do a separate little study of that, I think. That hasn't been done, at least not by us.

BR: With regard to the uniformity coefficient, have you thought about why it's as significant as it is? Conceptually, that is?

JS: I think initially at the time I wrote that paper, I just thought we don't have to know why. All we have to know is that it works. It's sort of like aspirin was for 50 years. Yes, I think there is some explanation in Sellmeijer's work about the importance of the D70/D10 ratio that he used. It has to do with the stability of the particles and the ease with which you can develop a quicksand condition.

BR: So the coarser particles are kind of armoring, essentially, the pipe?

JS: Haven't they found out with liquefaction that the higher the coefficient of uniformity, the less easily it will liquefy?

FT: Yes.

JS: So that's sort of the same thing. It sort of reinforces the liquefaction argument for piping. You start getting material with a higher coefficient of uniformity and then it's not going to liquefy and, therefore, not pipe as easily.

JS: Back to one of your questions way at the beginning. What did we learn? Well, I had always thought theoretically that there ought to be a big difference between scour velocities and piping velocities. With one series of tests, the ones in 1995, I got a chance to check that. Yes, there is a huge factor—50 to 100. That's something that I think is important to know. I gather from the scope of what you are planning to do with it that you were going to include investigating scour as well as piping. Correct?

BR: Yes.

FT: We had scour erosion happen. In some of the tests, we would increase the gradient so much that it would happen. In the report, we termed it sheet flow. The entire surface underneath the Plexiglas would just go, but the gradients were extremely high. I'd have to go back and read each test individually to find out when we had called it sheet flow.

BR: We saw that behavior in a couple of preliminary tests, which made us want a bladder in our setup as well. On a related note, in some of the tests you noted a head drop. The pipe would progress a ways and then your upstream reservoir head would drop. This even occurred in the Reid-Bedford sand. Do you think that this was because the 1/4-in. hose wasn't enough to sustain the head? If so, did that contribute to it coming to equilibrium and stopping?

FT: I honestly don't know.

BR: Okay.

FT: We had the manometers, which is a good idea, and religiously wrote down the manometer elevations. But, from my recollection, and you know I'm going back 30 years trying to remember, I don't recall using that data for anything. I think we did calculate an average

coefficient of permeability based upon the heads in the manometers, but that was all they were used for.

JS: Yes, we didn't make much use of the manometer data other than during the tests we would put a straightedge along there and you could see how uniform the gradient was. So, we would be checking that prior to the pipe forming.

BR: Did you use the manometers to dictate equilibrium in order to know when to increase the gradient?

FT: I don't know what we really used it for. During the moment of yelling back and forth and watching the piping, I can't recall exactly what the manometers did. I just don't remember if it was a big deal, but I would not build a flume without manometers.

BR: Absolutely... our flume has an array of 16 piezometers.

JS: Sixteen?

BR: Yes.

FT: I am old-fashioned. I would prefer the manometer over embedding a pore pressure transducer in there because that disrupts my continuity. I like an old-fashioned manometer. They work, and they're simple. With the little pore pressure transducers, I don't know if you have them saturated, if they're impeding the flow, or if the electronics are going to work.

BR: That's what we did. We put in a redundant system with an in-line 'T' so that you have a pore pressure transducer attached to each manometer.

FT: Oh, you have a transducer on the manometer? That's okay. I was thinking of embedding a pore pressure transducer in the sand. I would probably discourage that.

BR: I read through "The no-filter factor of safety against piping" [Schmertmann 2000] four times over the past few weeks. I don't believe I caught the significance of the paper the first couple of times I read through it. I think this paper was the first publication that really separated the seepage regime from the material response. Every other publication on the topic either averages the behavior at the site by looking at average gradient or does not account for the seepage characteristics at all. Along those lines, in your paper you created a series of correction factors to account for the different aspects of the flow nets that you used. If you are using a three-dimensional seepage analysis for these lab tests, those correction factors are inherent in your analysis, correct?

JS: You'll have to go back and look at the explanation for each of the correction factors and see whether a 3-D flow net would give you the answer such that you could eliminate that as a separate correction factor if you already have a 3-D seepage pattern. Yes, I would say there are some possibilities, but I would have to look at each one individually to see.

FT: If you have a numerical modeler that wants to use a 3-D finite element analysis, it would be interesting to see without doing a tremendous amount of work if they could duplicate the tests. I mean, I don't want to make a career out of running 3-D finite element analysis. They can bury you with reams of paper, but I would like you to say, "Okay, can you duplicate 1/10th of the tests we ran and tell me whether it's going to pipe or not pipe?" or something like that. As a practical guy, that is what I would like from the analysis. Do I have a problem or do I not have a problem? Then, the next step would be how bad of a problem do I have?

JS: I think Bryant is thinking about what I call the point method. That is, you are trying to follow the progress of a pipe to see if it will stop or if it will go all the way through. That's a very important question because I think that there are a lot of dams and dikes that have pipes in them that just got stopped.

FT: I am convinced of that.

JS: They didn't have the gradients to continue and then somebody decides, "Well, let's build the dam a little higher and put in some more water." Then, the gradients go up and the piping continues.

FT: I think they're just dormant out there waiting for the next flood. I mean, I think the whole Mississippi Valley probably is just sitting there waiting for the next flood. I think they progress and stop when the flood goes down. Then, the next flood kicks the progression off again.

BR: The point gradient idea now seems so simple to me. But, obviously it is not as it took me five times through your paper to really get it. Do you think that the difficulty in evaluating or even visualizing the three-dimensional seepage is what has prevented people from really grasping and using your paper?

JS: No, they didn't grasp the initial set of ideas. That is (1) a pipe can occur in any direction, not just vertically up like a boil, and (2) the concept that it's really a liquefaction problem. It's a micro-liquefaction problem or a micro-high gradient problem. Those are two of the key ideas or concepts, and you have to have that in mind. I had that in mind for 50 years but never got the opportunity to do tests. No one would pay for flume tests is what it amounted to, until the two failures – the Martin Dike, the Martin County Power Plant Project. Of course, there was big money involved in that so there was a lot of opportunity to do some research. Then, the same occurred with the IMC failure.

THE END OF INTERVIEW

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